June 24, 1956
Sunday

Hi Larry:

We just got back from Baltimore (by train) a few hours ago; I had not quite completed a longhand letter, but you wouldn't be able to read my calligraphy anyhow, so it's been done over. Esther's gone over the first part of the conference.

Altogether, it was no 7 day wonder. Jacob has a phenomenally detailed story on prophage genetics as well as sexuality, which I'm inclined to believe except for details; we have to thrash these out at Ann Arbor, where we hope to have more time together. I was pleased that we could get together in such a cordial spirit, and am going to do my best to keep it that way. What I could glean so far is that he's worked with 14 phages, of which 7 are inducible and Gal-linked; the other 7 are noninducible and in the Mal-Xyl region, rather resembling P1. They've more or less stayed away from transduction work, except of prophages. The 7 inducible phages have distinct lysogenic sites in K-12, but can be crossed to one another. They are also distinct in cross-immunity. The phage-determinant for site of fixation seems to be at the same (phage) locus as determines immunity reaction, and the same as a locus for clear-plaque mutation which determines ability (in lambda) to lysogenize at all. Jacob does most of his mapping in terms of "time of penetration" in interrupted fertilization experiments.

Harriett Eggrussi and Rollin both talked about quantitative titration of DNA per —x (after about 20 minutes each of what Jacob called 'jokes' and 'how to shake a tube'), with kinetics that was quite inconsistent, and useless in either case. Even with loglog plots they get, respectively:

but when I asked the innocent question of how many DNA molecules per cell and per —x, the answer was "but of course you can't answer that because the time during which cells remain competent is limited, and the DNA's interfere, etc. etc." And in any case, they could not demonstrate that the supernates lose any activity. So what are they measuring? From this I would conclude that radiation kinetics, fractionation work, etc are an absolute waste of time (though Bendich claims to have had some partial separation of different activities). Could you gently quiz Lerman about this, or at least warn him.

The Hemophilus system may prove cleaner, not Zamenhof's work which is impossibl unquantitative, but Goodgal and Herriott who are really digging in. It is fantastic that these studies still haven't reached the level of an acceptable M.S. thesis

We had a satisfactory tete-atete with Kalckar and Kurahashi, but he still let some of the catfur out of the bag. After Hartman misquoted the ms. about the PE relations of 1,4,7 and 2, I had to say that the former belonged to the same "cistron" (a possibly useful Benzerism for PE group) but that we steadfastly refuse to draw any conclusions so far. (Hartman's own talk as Esther mentions was simply awful; I never thought transduction could be made dull!)